An experiment investigating the spillover effects of communication opportunities

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Abstract

We report on an experiment designed to explore whether the effects of expressing one's emotions spill over into future interactions, thereby curtailing subsequent selfish decisions. In between two identical public goods games, participants play a binary-choice dictator game which, depending on the treatment, either gives or does not give the recipient the opportunity to text the dictator. The recipients of an unfair offer—in contrast to the recipients of a fair offer—contribute significantly less in the second public goods game. Yet, their contribution reductions are significantly smaller in the treatment allowing for recipient communication. To control for a belief-based explanation of these findings, we run treatments where we elicit beliefs about the other's contribution. We find that belief elicitation affects the efficacy of communication.

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1 Introduction

People experience negative emotions when they feel cheated, or when their partners do not live up to their expectations. Whether consciously or not, their emotions often affect the way they act.[1] Anger in particular tends to undermine trust and the ability to work closely together (Jehn, 1995; Allred et al., 1997).

Previous experimental research indicates that the possibility to voice one's emotions influences behavior within a given game. More specifically, it has been shown that if people are given the opportunity to express their disapproval of the others' choices, they reduce costly punishment in ultimatum games (Xiao and Houser, 2005) and increase cooperation in public goods games (e.g., Gächter and Fehr, 1999; Masclet et al., 2003; Rege and Telle, 2004; Noussair and Tucker, 2005). This paper adds a dynamic element to the analysis of the effects of communication opportunities: it investigates, through laboratory experiments, whether allowing the voicing of negative emotions—even to just acknowledge the other's misbehavior—affects future instead of current levels of cooperation.[2] Shedding light on this topic is important because if there are positive spillover effects from "having a voice", then the introduction of mechanisms designed to encourage the communication of emotions could help to re-establish cooperative attitudes among group members whose relations have become strained.

Work environments and personal relationships abound of examples where the experience of negative emotions endangers future cooperation between the involved parties. Co-authored papers, for instance, require effort by all participating researchers, and the free riding behavior by one of them could deter any form of collaboration with the free rider in the future. Although in real life we avoid acting selfishly toward people we know we are going to repeatedly interact with, it is undeniable that sometimes we do act in a self-interested way and hurt our partners, thereby triggering negative emotions in them. Actually, in our experiment, the participants are not aware that they will interact again with the same person at a later stage (although they know from the outset that the experiment consists of several stages). This resembles situations where, e.g., we would wish not to work again with colleagues that shirked in the past their responsibilities, but are forced to do so by external circumstances, such as a

[1] Elster (1998) provides a comprehensive survey study of the relationship between emotions and decision making. Battigalli et al. (2015) formally illustrate how anger and frustration shape social and economic interactions.

[2] We acknowledge that different negative emotions generate different behavioral predictions (see, e.g., Lerner and Keltner, 2000). However, determining which specific emotion (anger, resentment, irritation, or contempt) is experienced by our participants is beyond the scope of this study. Thus, we refer to negative emotions in general terms.

request from our principal.

Our approach involves letting participants play three games in the following order: a two-person public goods game, a binary-choice dictator game (or 'mini dictator game'), and one more public goods game that is identical to the first one. Following the standard practice in experimental economics (aimed at avoiding potential experimenter demand effects; see Zizzo, 2010), participants are informed from the outset that they will go through three stages and that they will learn about the content of each stage upon completion of the previous one. Pairs are reshuffled only between the first public goods game and the mini dictator game.[3] The dictator has to choose between a fair and an unfair offer. Participants do not receive any feedback on the outcome of the first public goods game until the second public goods game is completed,[4] while recipients are immediately informed about the outcome of the mini dictator game. The latter serves as a device for inducing, in the laboratory, negative emotions on the part of the recipients who have been treated unfairly. Such emotions could urge them to retaliate against selfish dictators (e.g., Nelissen and Zeelenberg, 2009; Clavien and Klein, 2010). We analyze the consequences of negative emotions on future cooperation by comparing the changes in contributions of the recipients who receive the unfair offer to the changes in contributions of the recipients who receive the fair offer. We assess the spillover effects of communication by comparing how the contributions of the unfairly treated recipients change in two different treatments: a treatment with a standard mini dictator game, and another treatment with a mini dictator game where the recipient, having learned of the dictator's allocation choice, is allowed to send him a text message.

Our analysis differs from existing work investigating the effects of ex post recipient communication in dictator games. Ellingsen and Johannesson (2008) and Xiao and Houser (2009) focus on the dictator's behavior and report that the prospect of verbal feedback motivates him to be fair. Conversely, we mainly look at the behavior of the other party, namely the recipient that got "justifiably angry" (Ellingsen and Johannesson, 2008, p. 101).[5]

[3]Since participants do not know in advance what the final stage will be, they are unaware of the fact that they will interact with the same person in the second public goods game. It may be claimed that this kind of incomplete information (to the extent that it is altering the participants' behavior by inducing them to be more selfish) constitutes deception by omission (see Krawczyk, 2013). While we cannot avoid possible misinterpretations on the part of the participants—that is the expectation of a new reshuffling of the pairs between the mini dictator game and the second public goods game—we worded the general instructions distributed at the outset in such a way that they could not possibly suggest such a reshuffling.

[4]This design feature prevents participants from experiencing, during the course of the experiment, emotions based on interaction in the first game.

[5]In a similar vein, Houser et al. (2012) analyze the behavior of recipients who were treated

Our conjecture is that, compared to recipients with no option of communication, recipients who can express themselves via symbolic gestures reduce their contributions less in the subsequent public goods game. In our experiment, two mechanisms can account for the spillover effects of messaging opportunities. Firstly, people may inherently value the chance to express their opinions (Katz, 1960; Ong et al., 2012), which does not necessarily mean using strong language. For instance, Tyler et al. (1985) find that the possibility to voice one's views heightens the feeling of justice and improves outcome satisfaction even when voice has no impact on outcomes. People often feel relief and satisfaction just because they can talk to the person who harmed them. Although never explicitly tested, it is conceivable that the relief of negative feelings associated with the opportunity to state one's case spreads to future interactions. In experimental studies manipulating mood states, the induction of a positive mood—via, e.g., film clips—has been found to increase people's generosity (Carlson et al., 1988; Isen, 2000; Kirchsteiger et al., 2006).

The second mechanism through which messaging opportunities may affect subsequent decisions is that they raise the recipient's expectations about the contribution of a selfish dictator in the second public goods game. An unfair offer in the mini dictator game could, indeed, lead the recipient to believe (once he has learned about the content of the final game) that the dictator, being selfish, will contribute nothing or very little in the second public goods game. If the recipient is a conditional cooperator (i.e., he contributes an amount that he thinks will be similar to the other's contribution; see Fischbacher et al., 2001; Fischbacher and Gächter, 2010) who has contributed a non-trivial amount in the first public goods game, he himself will contribute nothing or very little in the second public goods game, thereby reducing his own contribution. Yet, since verbal feedback may be expected to cause the dictator guilt or shame (Ellingsen and Johannesson, 2008; Xiao and Houser, 2009), the conditionally cooperative recipient may believe that the dictator will be more generous upon receipt of a message. Such a recipient will therefore reduce his contribution less in the presence than in the absence of messaging opportunities. To test for this belief-based explanation of the spillover effects of communication, we conduct treatments where we elicit subjects' beliefs about the other's contribution in the public goods games.

The evidence that we collected indicates that the recipients who receive the unfair offer in the mini dictator game, but not those who receive the fair one, tend to reduce their contributions in the final game. This difference can be

unfairly with the aim of assessing whether perceived unfairness relates to cheating behavior.

explained by the fact that recipients are conditional cooperators and expect the selfish (generous) dictators to contribute little (a fairly large amount) in the second public goods game. The observed reductions in the unfairly treated recipients' subsequent contributions are significantly smaller when these recipients are given the chance to send a text message to the dictators, but only in the treatments without belief elicitation. Incentivized belief elicitation is therefore found to affect the efficacy of communication.[6] When, as a robustness check of this result, we restrict the sample to the unfairly treated recipients who actually reduce their contributions, we observe that communication opportunities have a significant effect on how contributions change also in the presence of belief elicitation. A belief-based explanation for this latter finding is not supported by our data.

The paper is organized as follows. Section 2 introduces the games that constitute the basis of our experimental design. Section 3 is devoted to the design itself: it describes our treatments, states our research questions, and provides details on the employed experimental procedures. Section 4 reports our experimental results. Section 5 summarizes the main points of the study and offers concluding remarks.

2 The games

Each of our experimental sessions consists of a succession of three games: the first and third games are identical linear public goods games; the second game is a mini dictator game. Each participant therefore plays the final public goods game having acted as either a dictator or a recipient in the mini dictator game. We will refer to the participants as "dictators" or "recipients" depending on their role in the mini dictator game.

2.1 The mini dictator game

At the beginning of the mini dictator game (henceforth MDG) the participants are paired at random. Then the computer randomly determines, with equal probability, which pair member will act as the dictator. The dictator is offered €20 and the choice between two alternative allocations. The first one entails keeping €18 for himself and giving €2 to the recipient. The second allocation favors the recipient, albeit slightly; it gives €9 to the dictator and €11 to the recipient. We preferred this second allocation to the equal split so as to tempt

[6] Previous studies have shown that eliciting incentivized beliefs can significantly influence behavior (Croson, 2000; Gächter and Renner, 2010).

the dictators with the selfish decision and obtain a larger sample of recipients supposedly experiencing negative emotions after having received just €2.[7]

2.2 The two-person linear public goods game

To study the effect of messaging on future levels of cooperation, we rely on two identical linear public goods games, one played before the MDG and the other played immediately after the MDG. The first public goods game is the yardstick of the participants' cooperative attitudes.

In each public goods game (henceforth PGG), participants interact in pairs. Each pair member is endowed with €14 that he can either consume privately or contribute to the public good. Indicating a PGG by g, where $g = 1, 2$, and denoting the contribution of member i ($i = 1, 2$) in g by c_i^g, where $0 \leq c_i^g \leq 14$, i's monetary payoff in each g is given by:

$$\pi_i^g = (14 - c_i^g) + 0.75\,(c_1^g + c_2^g) \quad \forall\, i.$$

Since the marginal per capita return is less than unity, the dominant strategy for a monetary payoff maximizer is to contribute nothing. If both pair members free rode, then each one of them would earn €14. On the other hand, the socially efficient outcome is to contribute everything. If both pair members made the socially efficient choice, then each one of them would earn €21.

3 The experimental design

3.1 Treatments and research questions

The design manipulates two factors in a complete factorial design. The first factor refers to whether or not the recipient in the MDG—after being informed of his payoff—has the opportunity to send a written message to the dictator he is paired with. The second factor refers to whether or not beliefs about the other's contribution are elicited in the two PGGs. The characteristics of our treatments are summarized in Table 1. Each treatment is labeled with a sequence of letters. The first letter indicates whether or not the MDG allows for messaging opportunities (C stands for "control" and M for "message"). The remaining letter(s) indicate(s) whether or not beliefs are elicited in the PGGs (nB stands for "no beliefs" and B for "beliefs").

[7] Güth et al. (2001), for example, report that proposers in ultimatum games choose more often the unfair offer when the equal split is replaced by a nearly equal split that favors the responder. Charness and Rabin (2002) also show that people in allocation games avoid acts of generosity that result in being paid less than the others.

Table 1: Summary of experimental design.

Treatment	Recipients may send a message	Elicitation of beliefs in PGGs
CnB	NO	NO
MnB	YES	NO
CB	NO	YES
MB	YES	YES

As explained in Section 2.1, in the MDG of each treatment, the dictator chooses between two alternative allocations of €20. In the treatments with messaging opportunities (i.e., MnB and MB), each recipient—after viewing the offer—may text the dictator he is paired with; in the control treatments (i.e., CnB and CB), the recipient has no such possibility. These treatments are designed to shed light on the following questions:

Question 1 *Do the differences in contributions between the first and the second PGG (if any) depend on whether the recipients get €2 or €11 in the MDG?*

Question 2 *Do the recipients of €2 reduce (if at all) their contributions from the first to the second PGG less when they are allowed to send the dictators a message in comparison to when they have no such option?*

We address Question 1 by comparing, in all treatments, the differences in contributions between the first and the second PGG (that is $c_i^1 - c_i^2$) for the recipients of €2 to the same differences for the recipients of €11. We address Question 2 by comparing, for the recipients of €2, the differences in contributions between the first and the second PGG in each control treatment to the same differences in the corresponding message treatment. More formally, we compare $c_i^1 - c_i^2$ in CnB (CB) to $c_i^1 - c_i^2$ in MnB (MB), where i is restricted to the sample of unfairly treated recipients. We refer to the differences in contributions between the first and the second PGG as "contribution changes." Whenever these differences are positive, we talk of "contribution cuts."

As mentioned in Section 2.2, in both PGGs of each treatment, each pair member needs to decide how many out of €14 he wishes to contribute to the public good. In the treatments with belief elicitation (i.e., CB and MB), each pair member—after making his contribution decision—has one more task to perform: he must report what he expects his partner to contribute. We gave participants a financial incentive to report beliefs accurately. We paid them €3

for a belief that turned out to be correct, €2 for a belief that differed no more than one unit from the other's actual contribution, and nothing in all other cases. Incentives in the belief task were kept small relative to incentives in the contribution task in order to avoid hedging.[8]

Treatments CB and MB let us control for a belief-based explanation of the answers to Questions 1 and 2. Concerning Question 1, in comparison with the recipients of €11, the recipients of €2 may contribute (even) less in the second PGG in relation to the first one because they feel unfairly treated and experience negative emotions or because they are conditional cooperators and take the dictator's action as a signal of selfishness. Lower contributions in the second than in the first PGG cannot be ascribed to the outcome of the first PGG because participants do not receive feedback on this until the end of the experiment; nor can they be attributed to a desire to reestablish equality because participants (know that they) are paid according to their decisions in one game only so that income effects from the MDG can be excluded. As to Question 2, the reductions in the unfairly treated recipients' contributions could be smaller when messaging is permitted not only if messaging has a value in itself (Tyler et al., 1985; Ong et al., 2012), but also if it affects a conditionally cooperative recipient's beliefs about the dictator's future contribution in the sense that a message is expected to promote a feeling of guilt or shame in its receiver and consequently prompt him to act pro-socially (Ellingsen and Johannesson, 2008; Xiao and Houser, 2009).

We run treatments with and without belief elicitation in the two PGGs as there is evidence that eliciting beliefs—and, in particular, incentivizing belief elicitation—affects contribution decisions, although the direction of the effect is not clear in the literature (see, e.g., Croson, 2000; Gächter and Renner, 2010). The treatments with belief elicitation allow us to answer the following two questions:

Question 3 *Do unfair offers by the dictators serve as a signal of selfishness so that recipients who get €2 in the MDG expect their second PGG partner to contribute less than their first PGG partner and as a consequence (being conditional cooperators) lower their own contributions?*

Question 4 *Do unfairly treated recipients, acting in the belief that their message will induce dictators to cooperate more, reduce their expectations of their partners' contributions less when they can send a message in comparison to when they are not allowed to do so?*

[8] Blanco et al. (2010) show that hedging is not an issue in one-shot sequential prisoner's dilemma experiments insofar as the incentives to hedge are neither strong nor prominent.

We address Question 3 by comparing, for the recipients who get €2, their beliefs in the first PGG to their beliefs in the second PGG, and testing whether changes in beliefs are associated with analogous changes in contributions. We address Question 4 by comparing how the recipients who get €2 change their beliefs from the first to the second PGG in treatments CB and MB; formally, denoting i's beliefs in game g by b_i^g (where $i = 1, 2$ and $g = 1, 2$), we compare $b_i^1 - b_i^2$ in CB to $b_i^1 - b_i^2$ in MB.

3.2 Procedures

The experiment was programmed in z-Tree (Fischbacher, 2007) and conducted in the experimental laboratory of the Max Planck Institute of Economics (Jena, Germany). The participants, undergraduate students from the Friedrich Schiller University of Jena, were recruited using Greiner's (2015) ORSEE software. They had never participated before in either a social dilemma experiment or a dictator game experiment.

We used a between-subjects design, i.e., each subject was exposed to only one of the four treatments. Overall, we ran twenty-three sessions with a total of 716 participants. The fourteen sessions devoted to the CnB and MnB treatments (with 30 to 32 participants per session) took place from the 7th of September 2011 to the 5th of December 2011; those devoted to the CB and MB treatments (with 24 to 32 participants per session) were run one year later, from the 12th of November to the 3th of December 2012.

In each session, each of the three games was presented separately at a different stage of the experiment.[9] Although participants knew from the beginning that there would be three stages, each participant learned about the content of each stage only after having completed the previous one.[10] All games were run one-shot. We implemented a stranger matching protocol between the first PGG and the MDG, and a partner matching protocol between the MDG and the second PGG. We cannot rule out the possibility that some dictators behaved selfishly in the MDG under the (false) impression that they would not be rematched with the same person in the final game. However, nothing in the general instructions distributed at the outset indicated that there would be such a rematching. In fact, the general instructions did not give any hint about the content of the third stage, which from the participants' perspective could

[9] The full sequence of events is set out in Appendix A in the Online Supplement. The latter also contains (see Appendix D) a translation of the instructions (originally in German) for the MB treatment.

[10] Besides minimizing experimenter demand effects (Zizzo, 2010), this design feature prevents the recipients from using the text message to communicate how the dictator should behave in the final public goods game.

have been an individual decision-making setting or any other setting that does not require cooperation (e.g., a voting game). We are therefore confident that there was no deception by omission (Krawczyk, 2013) in our experiment.

In treatments MnB and MB, each recipient was given four minutes to type his message in a text box, but it was at his discretion to send it ahead of the deadline. The form of the message was free; the only restriction to its content was that its sender could not identify himself. The dictator knew that the recipient would have the possibility to send a message.

To minimize path dependence between the two PGGs (i.e., the dependence of choices in the second PGG on outcomes in the first PGG), as well as learning effects (see Andreoni, 1988), subjects received feedback about the other's contribution and their own payoff in the first PGG only after having completed the second PGG. To avoid income effects, one of the three games was chosen at random and subjects were paid according to their decisions in that game (subjects knew about this procedure since the beginning of the session).

In all treatments, after interaction in the second public goods game, we had recipients report the emotion, if any, they experienced when they found out the dictator's decision. Recipients had to select one among the following eleven emotions: pride, envy, anger, happiness, shame, irritation, gratitude, surprise, contempt, admiration, or none.[11]

Each experimental session lasted about an hour. Averaging over all sessions, mean earnings amounted to €17.85 (inclusive of a €2.5 show-up fee).

4 Experimental results

4.1 Assessing the quality of randomization across comparable treatments

First, we discuss the extent to which participants were assigned to treatments in a representative way.[12] Figure 1 draws, separately for each treatment, boxplots of all subjects' contribution choices in the two PGGs (PGG 1 refers to the first public goods game and PGG 2 to the second, the $\times$ symbol denotes the mean). With unbiased recruitment, it should not be possible to reject the null hypothesis that the PGG 1 contributions in treatments CnB and MnB, as well as in treatments CB and MB, have identical distributions. Wilcoxon rank sum tests indicate that this is indeed the case (p-value = 0.62 for CnB vs. MnB,

[11] Averaging across all treatments, 69% of the recipients who got €2 did select a negative emotion. The list contains both negative and positive emotions for two reasons: (i) we did not want to push participants in a particular direction; (ii) we expected recipients getting €11 to report a positive emotion.

[12] Unless otherwise stated, all statistical tests presented in this section are two-sided.

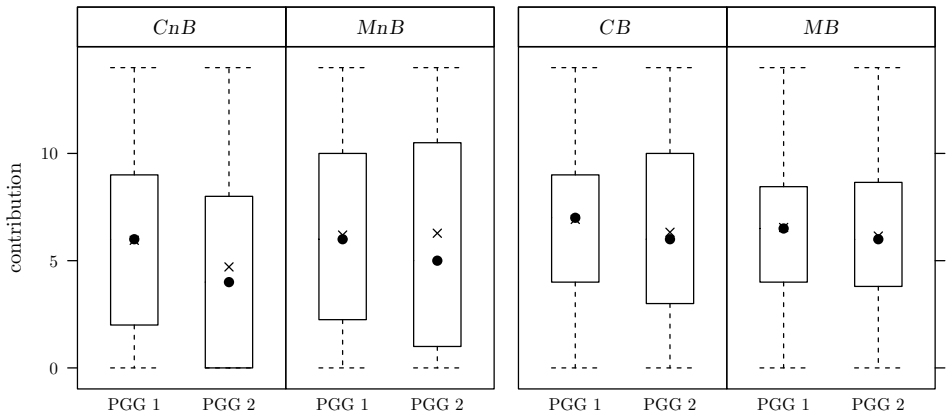

Figure 1: Boxplots of contributions in the first and the second public goods game (PGG 1 and PGG 2, respectively) per treatment.

0.27 for CB vs. MB). We can therefore conclude that randomization worked when comparing PGG 1 contributions between treatments that manipulate the presence versus absence of messaging opportunities.

In line with the results of Gächter and Renner (2010), contributions are larger in the treatments with incentivized belief elicitation than in the no-belief treatments; the difference is significant for the comparison between CnB and CB, but not for the comparison between MnB and MB (p-values equal 0.01 and 0.30, respectively; Wilcoxon rank sum test).

Table 2 categorizes our subjects according to their role and earnings in the MDG and reports the average contribution of each category in each of the two PGGs (the relative frequencies and further descriptive statistics of the contributions can be found in Figure C1 in the Online Supplement).

We examine whether recipients of €2—the main object of our analysis—differ in their contributions to the first public good across treatments. According to Table 2, these contributions average 6.31 (7.21) in CnB (CB) and 5.79 (7.03) in MnB (MB). No significant difference is detected between either CnB and MnB (p-value = 0.53; Wilcoxon rank sum test) or CB and MB (p-value = 0.67). Additionally, there is no significant difference between pairs of treatments that differ only with respect to belief elicitation (p-value equals 0.21 for CnB vs. CB and 0.11 for MnB vs. MB; Wilcoxon rank sum test). Hence, the unfairly treated recipients are sufficiently homogeneous in terms of cooperative attitudes across comparable treatments.[13]

[13]The significant difference in the PGG 1 contributions between CnB and CB that we detected earlier at the population level is attributable to the dictators who contribute signifi-

Table 2: Average contribution in the two PGGs, separately for each type of participant and each treatment.

	CnB treatment			MnB treatment			CB treatment			MB treatment		
	N[a]	PGG 1	PGG 2	N	PGG 1	PGG 2	N	PGG 1	PGG 2	N	PGG 1	PGG 2
All participants	222	5.95	4.71	220	6.19	6.28	154	6.92	6.32	120	6.55	6.15
▷ Recipients	111	6.35	4.32	110	6.20	6.24	77	6.58	5.99	60	6.79	6.11
¬ Getting €2	82	6.31	3.06	64	5.79	4.45	47	7.21	5.48	42	7.03	5.30
– with $c_i^2 < c_i^1$	44	7.76	1.26	26	6.10	2.00	25	7.62	3.37	27	6.27	3.23
– with $c_i^1 = c_i^2$	31	4.53	4.53	32	5.50	5.50	16	7.34	7.34	12	8.79	8.79
– with $c_i^2 > c_i^1$	7	5.10	7.87	6	5.92	9.50	6	5.15	9.30	3	6.83	10.00
¬ Getting €11	29	6.46	7.88	46	6.77	8.73	30	5.59	6.79	18	6.23	7.99
▷ Dictators	111	5.55	5.10	110	6.19	6.31	77	7.27	6.66	60	6.30	6.18
¬ Keeping €18	82	4.62	3.88	64	4.84	4.63	47	7.11	6.09	42	5.53	5.08
– with $c_i^2 < c_i^1$	27	6.33	3.02	19	5.50	2.91	22	7.37	3.63	13	7.60	4.25
– with $c_i^1 = c_i^2$	42	3.62	3.62	34	4.85	4.85	15	8.00	8.00	18	4.22	4.22
– with $c_i^2 > c_i^1$	13	4.28	6.54	11	3.68	6.95	10	5.18	8.62	11	5.24	7.46
¬ Keeping €9	29	8.17	8.53	46	8.07	8.65	30	7.52	7.55	18	8.11	8.77

[a] N stands for the number of subjects in each category.

4.2 Spillover effects of messaging opportunities

In what follows we provide an answer to our first two research questions. As to Question 1 (asking whether the recipients of €2 and €11 differ with respect to their contribution changes), Table 2 shows that, whatever treatment we consider, the recipients of €2 (€11) contribute, on average, substantially less (more) in the second PGG than in the first one. In all treatments, the contribution changes are significant for both types of recipients (p-value < 0.02 for all comparisons and irrespective of the amount got by the recipient; Wilcoxon signed rank test), and so is the difference in contribution changes between the recipients getting €2 and the recipients getting €11 (p-value < 0.01 for all comparisons; Wilcoxon rank sum test).[14] These results provide an affirmative answer to Question 1 and allow us to state:

Result 1 *In all treatments, the recipients who get €2 contribute significantly less in the second PGG than in the first one, whereas those who get €11 contribute significantly more.*

Question 2 asks whether the contribution changes of the recipients getting €2 are affected by the possibility of sending a message. Table 2 suggests that it is only when beliefs are not elicited that these recipients reduce their contributions to a smaller extent in the treatments with messaging opportunities than in the control treatments ($c_i^1 - c_i^2$ equals, on average, 1.34 in MnB and 3.25 in CnB; $c_i^1 - c_i^2$ equals 1.73 in both MB and CB). There is a significant difference in contribution changes between CnB and MnB (p-value = 0.03, one-sided Wilcoxon rank sum test), but not between CB and MB (p-value = 0.65). Thus messaging opportunities have spillover effects in the absence, but not in the presence, of incentivized belief elicitation.

Compared to the unfairly treated recipients who either do not change or even increase their contributions, the unfairly treated recipients who reduce their contributions presumably appreciate more the introduction of communication opportunities. Returning to the example of co-authored papers given in the Introduction, it is exactly when we do not wish to work again with colleagues who shirked in the past their responsibilities that the need for communication is felt more urgently and the possibility of communication could prove decisive

cantly different amounts in these two treatments (p-value < 0.01, Wilcoxon rank sum test; as to the remaining pairwise comparisons, the p-values are 0.40 for CnB vs. MnB, 0.11 for CB vs. MB, and 0.64 for MnB vs. MB).

[14] Since the recipients who get €11 increase their contributions from the first to the second PGG, it is clear that the subsequent lower contributions of the unfairly treated recipients cannot be attributed to learning how to play the free riding equilibrium (in the sense of Andreoni, 1988).

in helping us to change our mind and eventually start a new collaboration.

Hence, as a robustness check, we repeat the above analysis restricting this time our sample to the unfairly treated recipients who actually reduce their contributions (i.e., recipients with $c_i^2 < c_i^1$). One-sided Wilcoxon rank sum tests show now that the difference in contribution cuts is significant at the 5% level for the comparison CnB vs. MnB (p-value = 0.04) and at the 10% level for the comparison CB vs. MB (p-value = 0.08).[15] We summarize the results on Question 2 as follows:

Result 2 *Messaging opportunities reduce contribution cuts significantly in all treatments if we consider the subsample of unfairly treated recipients who reduce their contributions. With respect to the entire sample of recipients of €2, significant spillover effects of messaging opportunities are confined to the treatments without belief elicitation.*

4.3 Analysis of the elicited beliefs

We now turn to the analysis of the elicited beliefs in order to (i) investigate whether recipients of €2 behave as if they were conditional cooperators, and (ii) assess whether it is conditional cooperation or the possibility of "having a voice" per se that accounts for the spillover effects of messaging opportunities observed for all recipients of €2 in the treatments without belief elicitation, and for the subsample of unfairly treated recipients who reduce their contributions in all treatments. Table 3 provides the means and standard deviations of the elicited beliefs per type of participant and treatment.

Starting from the first part of Question 3 (asking whether the dictators' unfair offers signal selfishness to the recipients), in both treatments with belief elicitation the recipients of €2 expect from their partners significantly smaller contributions in the second PGG than in the first one (p-value < 0.01 for both CB and MB; Wilcoxon signed rank test). The opposite holds for the recipients of €11; they expect higher contributions in the second PGG than in the first one, albeit significantly so only in the CB treatment (p-value < 0.01 for CB and p-value = 0.32 for MB; Wilcoxon signed rank test). The null hypothesis that, in the second PGG, there is no difference in stated beliefs between the recipients getting €2 and the recipients getting €11 can be rejected (p-value = 0.048 for CB and 0.03 for MB; Wilcoxon rank sum test). Consequently,

[15] The percentage of unfairly treated recipients who do not change or increase their subsequent contributions in CB (46.81%) compared to MB (35.71%) may explain the finding of non-significant difference that we detected at the population level.

Table 3: Descriptive statistics of elicited beliefs, separately for each type of participant and each treatment.

	CB				*MB*			
	PGG 1		PGG 2		PGG 1		PGG 2	
	mean	s.d.	mean	s.d.	mean	s.d.	mean	s.d.
All participants	7.21	3.18	5.96	3.86	6.26	3.24	5.50	3.95
▷ Recipients	6.84	3.33	6.33	3.73	6.60	3.12	5.75	3.66
¬ Getting €2	7.65	3.41	5.78	3.90	6.53	2.95	5.01	3.28
¬ Getting €11	5.57	2.80	7.20	3.33	6.78	3.57	7.46	4.02
▷ Dictators	7.58	3.01	5.59	3.96	5.92	3.35	5.25	4.24
¬ Keeping €18	7.24	3.06	4.15	3.50	5.29	3.05	3.67	3.62
¬ Keeping €9	8.10	2.89	7.86	3.61	7.39	3.63	8.94	3.18

recipients do take a selfish action in the MDG as a sign that their partner will be selfish in the future.

Concerning the second part of Question 3 (asking whether the behavior of the unfairly treated recipients is consistent with conditional cooperation), we note that the unfairly treated recipients' changes in the amount they expect their partners to contribute are positively and significantly correlated with their own contribution changes ($\tau = 0.36$ with p-value < 0.01 for CB and $\tau = 0.62$ with p-value < 0.01 for MB; Kendall's rank correlation coefficient). These results indicate that we can answer Question 3 affirmatively, that is:

Result 3 *Unfair offers in the MDG serve as a signal of selfishness and the way the unfairly treated recipients adjust their contributions from the first to the second PGG is consistent with their belief changes, in other words they behave as if they were conditional cooperators.*

Conditional cooperation appears to be a likely explanation of behavior also for the favorably treated recipients as the increase in their beliefs about the other's contribution is positively correlated with the increase in their own contribution (Kendall's τ between $b_i^2 - b_i^1$ and $c_i^2 - c_i^1$ equals 0.46, with a p-value less than 0.01, for CB and 0.41, with a p-value of 0.02, for MB).

As to Question 4 (asking whether unfairly treated recipients reduce their expectations of their partners' contributions less in the presence of messaging opportunities), a one-sided Wilcoxon rank sum test comparing $b_i^1 - b_i^2$ in CB

to $b_i^1 - b_i^2$ in MB does not confirm that the recipients of €2 reduce their contribution expectations to a smaller extent when they are given the opportunity to write a message to their partners (p-value = 0.60). The same holds if we restrict our attention to the unfairly treated recipients with decreasing contributions (p-value = 0.72 by the same test). It appears that the latter substantially reduce their contribution cuts in the presence of messaging opportunities even if they do not believe that the selfish type of player they are paired with will become less uncooperative (because of their message) in the subsequent interaction.

Result 4 *The unfairly treated recipients do not reduce their expectations of their partners' contributions less when they can send a message in comparison to when they are not allowed to do so.*

The increase in contribution expectations of the recipients who get €11 is higher when messaging is not permitted: the mean of $b_i^2 - b_i^1$ equals 1.63 in CB and 0.68 in MB, but the difference is not significant (p-value = 0.54, Wilcoxon rank sum test).

4.4 Self-reported emotions and the messages' content

Table 4 presents, separately for the recipients who got €2 and the recipients who got €11, the results of the post-experimental questionnaire, that is how the recipients described their own feelings upon learning the dictator's decision. Most of the unfairly treated recipients self-reported the feeling of a negative emotion; the exact percentages per treatment are as follows: 69.5% in CnB, 68.8% in MnB, 72.3% in CB, and 64.3% in MB.[16] The percentage of those claiming to have felt no emotion (that is "none" in the classification of Table 4) ranges from 14.9% in CB to 26.2% in MB. Very few unfairly treated recipients (3.7% in CnB, 6.3% in MnB, 2.1% in CB, and 0.0% in MB) reported a positive emotion. Overall, 73 out of the 235 recipients getting €2 (i.e., 31.1%) did not report a negative emotion. We note that the results presented above for the entire sample do not qualitatively change if we exclude these 73 recipients from the analysis. The only difference is that the result of the one-sided Wilcoxon rank sum test investigating whether messaging opportunities lead to smaller contribution changes (our Question 2) is significant at the 10% level for the CnB vs. MnB comparison (p-value = 0.09).

Table 5 classifies the messages written by both types of recipients on the basis of their emotional content (the methodological details are given in Ap-

[16] Similarly, between 69.0% and 88.9% of the recipients that were offered €11 self-reported the feeling of a positive emotion.

Table 4: Relative frequencies of the recipients' self-reported emotions.

Emotion	recipients getting €2				recipients getting €11			
	CnB	MnB	CB	MB	CnB	MnB	CB	MB
Anger	0.06	0.14	0.04	0.00	0.00	0.00	0.00	0.00
Irritation	0.44	0.33	0.51	0.52	0.00	0.00	0.00	0.00
Contempt	0.12	0.08	0.11	0.02	0.00	0.00	0.00	0.00
Envy	0.07	0.14	0.06	0.10	0.00	0.00	0.00	0.00
Shame	0.01	0.00	0.00	0.00	0.00	0.02	0.00	0.00
Surprise	0.01	0.05	0.11	0.10	0.28	0.17	0.20	0.11
None	0.24	0.20	0.15	0.26	0.03	0.07	0.00	0.00
Happiness	0.01	0.02	0.02	0.00	0.10	0.17	0.37	0.06
Gratitude	0.00	0.00	0.00	0.00	0.41	0.43	0.20	0.44
Admiration	0.00	0.05	0.00	0.00	0.14	0.09	0.20	0.28
Pride	0.02	0.00	0.00	0.00	0.03	0.04	0.03	0.11

Note: The first (last) four emotions are negative (positive), the remaining three emotions are neutral.

Table 5: Message classification in the two treatments with messaging opportunities.

		Negative	Neutral	Positive
Treatment MnB	recipients getting €2	0.35	0.59	0.06
	recipients getting €11	0.02	0.00	0.98
Treatment MB	recipients getting €2	0.52	0.43	0.05
	recipients getting €11	0.00	0.11	0.89

Note: In MnB (MB), 64 (42) recipients received €2 and 46 (18) recipients received €11. In each treatment, one recipient with €2 did not send any message.

pendix B in the Online Supplement). In each of the two treatments with messaging opportunities, all recipients except one sent a message to their dictators. In treatment MnB, 63 messages were written by recipients receiving the unfair offer; 35% of them were classified as expressing negative emotions and the majority (namely 59%) as expressing neutral emotions. In all but a few neutral messages, the recipient did rebuke the dictator for his choice, but also confessed

that he would not have acted differently had the right of choice been given to him. For example, one such neutral message refers to the dictator's choice as stupid: "I cannot do anything else but accept your choice. I find it stupid that you did not give me more, but I would have acted the same way." And another message classified as neutral says: "Hi Mr./Mrs. Unknown, I pity you for your choice, but probably I would have done the same." The frequency of this kind of neutral message is lower in treatment MB, where 52% of the 41 messages written by the recipients getting €2 are classified as expressing negative emotions.[17] Only 6% (5%) recipients getting €2 in MnB (MB) sent a message that was classified as showing positive emotional content.

With regard to the messages written by recipients getting €11, 98% (89%) of them were classified in MnB (MB) as having positive content.

As a final remark, we note that our results rely on the assumption that the unfairly treated recipients felt negative emotions. We strongly believe that the recipients' self-reported emotions provide more accurate information about their emotional state as compared to the classification of the messages' content because, in the words of Xiao and Houser (2005, p. 7401), such a classification cannot reveal "the true emotion behind any of the messages we collected". The fact that many unfairly treated recipients identified with selfish dictators, which most likely led the evaluators to independently classify their messages as neutral, does not necessarily imply that they did not experience a negative emotion.[18]

4.5 Dictator behavior

We conclude this section by briefly reporting on the dictators' behavior. First we note that the percentage of selfish dictators in the MDG (65.8% across all treatments) is higher compared to earlier studies of mini dictator games. Most of these studies let the dictators choose between an equal split and a split that is favorable to them, and find that about one quarter of the dictators behave selfishly (see, e.g., Kahneman et al., 1986; Dana et al., 2007; or the survey by

[17] As suggested by an anonymous referee, a possible explanation for the difference in the frequency of negative messages between MnB and MB is that the elicitation of beliefs in the first PGG influences the emotional state of the unfairly treated recipients in a way that they want to air more grievances against their dictator. Result 2 above hints at the possibility of a relationship between belief elicitation and emotion.

[18] When we retest Question 2 considering the unfairly treated recipients who (i) self-reported to have experienced a negative emotion upon learning the dictator's decision and (ii) sent a message whose content was classified as negative, the p-value of the one-sided Wilcoxon rank sum test is 0.24 for CnB vs. MnB and 0.25 for CB vs. MB. This result is most likely attributable to the small size of the subsample (14 observations for MnB and 18 for MB), which may determine a low power of the test and make its result highly prone to a type II error.

Camerer, 2003, p. 56).[19] Our result validates the choice of an allocation that is slightly favoring the recipient—rather than the equal split—so as to tempt the dictators with the selfish choice.

Table 2 and Figure C1 in the online supplement show that, in the treatments without belief elicitation, there are fewer dictators choosing the selfish allocation when text messaging is permitted than when it is not permitted (58.2% in MnB vs. 73.9% in CnB). The difference in frequencies is significant at the 1% level (p-value = 0.01, test for equality of proportions). Conversely, in the treatments with belief elicitation, the percentage of selfish choices by the dictators is higher—though not significantly higher—in the presence than in the absence of messaging opportunities (70.0% in MB vs. 61.0% in CB; p-value = 0.27, test for equality of proportions). Hence, our findings are consistent with those of Ellingsen and Johannesson (2008) and Xiao and Houser (2009) only for the treatments without beliefs.

Turning to the analysis of the dictators' contribution behavior, Table 2 reveals that, in all treatments, most of the selfish dictators (84.1% in CnB, 82.8% in MnB, 78.7% in CB, and 73.8% in MB) either do not modify or reduce their contributions between the first and the second PGG. We note, however, that PGG 2 contributions are significantly lower than PGG 1 contributions in the control treatments (p-value < 0.01 for CnB and 0.06 for CB, Wilcoxon signed rank test), but not in the treatments where selfish dictators receive a message (the Wilcoxon signed rank test p-value is 0.22 for MnB and 0.56 for MB). This holds independently of whether the selfish dictators receive a message classified as negative (p-value = 0.30 for MnB and 0.24 for MB) or a message classified as either neutral or positive (the respective p-values are 0.54 and 0.96).

As regards generous dictators, Wilcoxon signed rank tests indicate that upon receipt of a positive message they significantly increase their contributions (p-value = 0.06 for MnB and 0.05 for MB).[20] No significant difference between PGG 1 and PGG 2 contributions is detected in the control treatments (p-value = 0.75 for CnB and 0.69 for CB).

[19] Dana et al. (2007), for instance, in their baseline treatment give the dictators a choice between an equal split of \$10 and an unfair and welfare inefficient option yielding \$6 to the dictator and \$1 to the recipient. They observe that merely 26% of the dictators choose the unfair option.

[20] The reader is reminded that 98% (89%) of the messages written by recipients getting €11 in MnB (MB) are classified as positive.

5 Conclusions

Numerous psychological and economic experiments have shown that emotions play a part in decision-making. Laboratory research has also documented that, within a given environment (usually an ultimatum or a public goods game), symbolic non-monetary punishment serves as a deterrent to costly and inefficient actions. What is novel in the present paper is that we link the experiencing of negative emotions and the presence of communication opportunities in one game to behavior in a subsequent game. Practically speaking, we test for spillover effects.

The contribution rates of the recipients of an unfair offer show clear signs of dropping off both in the presence and in the absence of communication opportunities. This is reminiscent of the results of Houser et al. (2012), who found that people reporting to have been unfairly treated in a dictator game were more likely to cheat in a subsequent coin flip experiment. In both their experiment and ours, the experience of unfair treatment reduces the participants' willingness to act pro-socially.

Although we showed that the subsequent lower contributions of the unfairly treated recipients cannot be attributed to learning,[21] it could be explained in terms of conditional cooperation as the changes in contributions are found to be positively correlated with the changes in stated beliefs. More specifically, players who receive a small offer in the dictator game lower their beliefs about the other's contribution and become less cooperative in the future.

With respect to the entire sample of unfairly treated recipients, we find evidence of the spillover effects of messaging opportunities only when beliefs are not elicited: in the treatments with belief elicitation, the unfairly treated recipients who have the possibility of sending a message to the dictator do not reduce their subsequent contributions significantly less in comparison with the unfairly treated recipients who do not have such a possibility. This result may be attributable to the fact that, in the case of belief elicitation, the percentage of unfairly treated recipients who do not change or even increase their contributions is higher in the control (47%) than in the message treatment (36%).

When we restrict our sample to the unfairly treated recipients who actually reduce their contributions from the first to the second public goods game, then we observe that the presence of messaging opportunities leads to the cur-

[21] If players had a better understanding of the dominant strategy while playing the second public goods game, then the decrease in contributions should be universal to them all. We observed, instead, that the recipients getting €11 contribute, on average, more in the second public goods game than in the first one.

tailment of contribution cuts in all treatments, those with and without belief elicitation. As stated elsewhere in the paper, in comparison with the unfairly treated recipients who—in the absence of communication opportunities—do not change or even increase their contributions, the unfairly treated recipients who reduce their contributions may be more appreciative of the introduction of communication channels.

As to the mechanism accounting for the spillover effects of messaging opportunities, our data do not support the hypothesis that the observed smaller reductions in contributions are driven by conditionally cooperative recipients who expect their partners to contribute more following the receipt of their message. Having formed a negative perception of the partner, the unfairly treated recipients are left with no faith on a written message as corrective action. The finding that the reductions in contributions, but not in beliefs, are smaller in the treatments with messaging opportunities than in the control treatments indicates that people inherently value the chance to express their opinions (as claimed by, e.g., Katz, 1960; Ong et al., 2012), though the content of the messages was not always classified as conveying negative emotions.

An important lesson to be learned from our experiment is that eliciting incentivized beliefs (which is known to affect contribution decisions) appears to influence the emotional state of the unfairly treated recipients. Messaging opportunities become less important to them. In other words, despite the absence of messaging opportunities the unfairly treated recipients are less likely to reduce their subsequent contributions. While the data presented here do not allow us to ascertain the source of the relationship between belief elicitation and relief of negative emotions, they do suggest that the elicitation of beliefs is related to emotions in some way.

As a final remark, we employed a computer-mediated communication mode to isolate the impact of written emotional expressions from visual and verbal stimuli. Given this setting, the question arises as to whether our findings generalize to situations where emotions can be expressed verbally or through gestures. Further research is needed to explore this issue as well as the mechanisms linking belief elicitation to the relief of negative feelings.

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